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the body, *e. g.*, or some part or action of it. And, where 'you, I and the chair,' *e. g.*, are all objects of one sensation, 'that identical chair may be your object and my object'; it is then 'essentially public.' On the other hand, hallucinations and dreams, and in part at least, I suppose, a feeling as of hunger or thirst, which are not objects in a world of experience where I and the rest of us stand in similar relations to them, these are private objects. This distinction is 'strictly empirical'; and idealism, since it is based on the essentially private character of *all* experience of objects, 'dissolves away like the architecture of dreams.'

But it seems to me that, though we grant that the subject of consciousness is ever an object also, yet when we turn to the meaning and test of the distinction between truth and error, Dr. Bush himself shows that the way to idealism and the absolute is just as open and easy as before, so long as we speak of *private objects* 'streaming' (even though they do not 'exist') in consciousness (p. 567). For when he says that 'the actual test whether my visual object be chair or hallucination would be to find whether you too see what I do,' and that 'error in science is the fact of rejection by other observers,' he starts with the existence of a private object (visual or other, mine or another's) and asks whether it be public or private only. So that, with this private object as the grammatical and logical subject of the inquiry, whatever predicate (hallucination or reality) may be found for it, to it alone existence, within the range of the inquiry, can primarily pertain. Hence may easily follow the conclusion that 'true' existence, whatever *test* we apply, *is* and means the private experience of an absolute.

Is not the distinction between private and public objects just the distinction between consciousness of an object and the object which Dr. Bush starts by rejecting? Is not my dream, in so far as it is an object, public in the same sense as the chair, though it is not studied by me and by others in the same way, while all may, if equally near, etc., study the chair in the same way?

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REVIEWS AND ABSTRACTS OF LITERATURE

La Valeur de la Science. H. POINCARÉ Paris, Ernest Flammarion. 1905. Pp. 278.

A hopeful sign for present-day philosophy and psychology is the persistence with which the common sense of the οἱ πολλοί is steadily creeping into the somewhat attenuated and rarefied regions of academic thought. Emphasis even in philosophy is being placed more and more upon the scientific treatment of data, upon the tentative character of our knowledge and hypotheses, and upon the possibility of the widening glimpses which may be had in the realms of experience. To quote Tennyson in this connection,

> "Yet all experience is an arch where thro'
> Gleams that untravell'd world, whose margin fades
> For ever and for ever when I move."

M. Poincaré well illustrates this empirical tendency in his small volume. I should hesitate to call his book 'pragmatic,' still I think that its general tendency is in that direction. It may be advisable, therefore, to orient any possible reader by pointing out the 'pragmatic' tinges in it before touching upon the contents in general. I have, in the following quotations, italicized the words that indicate the pragmatic tendency.

Concerning time and space we are told: "It is not nature which imposes them on us, it is we who impose them on nature, because we find them *convenient*" (p. 6). "The law of Newton is a truth of experience; as such, it is only *approximate,* and shows us that as yet we have only a *tentative* definition. . . . There is only one way of measuring time, which is truer than any other; that which is generally adopted is solely that which is most *convenient*" (p. 44). Moreover, any affirmations one way or the other in this connection can have no meaning *per se.* "They can have meaning, only through *convention*" (p. 46). Similarly concerning scientific rules. "We choose rules, not because they are true, but because they are the most *convenient*" (p. 57). The three dimensions of space, so-called, are not such *per se.* "Experience does not prove to us the three-dimensional character of space; rather it shows us that it is *convenient* for us to attribute three dimensions to it, because by this means any possibility of error is reduced to a minimum" (p. 125). Concerning science, we do not say that it is useful because it teaches us how to build machines. Rather "machines are useful because they do our work, and so enable us to spend more time at scientific investigation" (p. 166). To which I may add that, therefore, science is useful because it enables us to do our work in a more expeditious and agreeable manner. Our basis in all cases is experience. "The scientific fact is nothing more than the brute fact translated into more *convenient* language" (p. 231). Again, "Every law is only an imperfect and *provisional* formulation which must, in the future, be replaced by another higher law, of which, at present, we have but the roughest conception" (pp. 251, 252).

So much for these spots of light. Concerning the contents[1] of the book in general, the variety of subjects is somewhat too great to be adequately treated. M. Poincaré, however, writes in a most concise manner, and, unlike the average Frenchman, is no easy reading. His remarks are pointed, and his whole treatment is rather a series of careful analyses, than a systematic and connected treatise. As the table of contents shows, the outline of topics is rather ambitious for so small a volume.

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[1] Contents are: Part I., The Mathematical Sciences: I., Intuition and Logic in Mathematics; II., The Measure of Time; III., The Notion of Space; IV., Space and its Three Dimensions. Part II., The Physical Sciences: V., Analysis and Physics; VI., Astronomy; VII., History of Applied Mathematics; VIII., The Crisis in Physics; IX., The Future of Applied Mathematics. Part III., The Objective Value of Science: X., Is Science Artificial? XI., Science and Reality.

In contrasting the values of intuition and logic, M. Poincaré asserts that the former does not give us certitude, while the latter does, and hence the emphasis upon the latter in scientific demonstration. It seems to me, however, that intuition is the basis of all reasoning, affording us the halting-places in the upward passage of logical advance. The verbal and other symbols have no meaning without the 'set' given them by the attitude which accompanies them, without the intuitional response acting as a step of the ladder in the progress of the thought concerned. And concerning the certitude of the intuitional or 'geometrical' method in mathematics, surely Newton's *Principia* are just as valid as the more 'logical' treatment of Leibnitz.

In the treatment of space, we find an original discussion of the continuum in its various dimensions. Space, as a pristine experience, is amorphous. Upon this as a basis, we develop the various continua. In brute experience, we may be unable to distinguish, in a physical continuum, two impressions, while we may be able to tell each of them from some third, higher or lower in the scale. For example, while we may be able to distinguish 10 *gr.* from 12 *gr.*, still 10 *gr.*, 11 *gr.*, 12 *gr.*, etc., may not be so distinguishable. This gives us some idea of a continuum in our original experience. As a *felt* experience, we may express this by the formula,

$$A = B, \qquad B = C, \qquad A < C,$$

thus symbolizing a continuum as experienced. We may, however, have a continuum of one or more dimensions, and here the notion of division, breach or break must be introduced.

We may represent the parts of a physical continuum by

$$E_1, E_2, \cdots, E_n,$$

which is so *experienced* that (1) all the parts are felt as belonging to the continuum, and (2) each is indiscernible from the following. When, to divide a continuum, it is necessary to consider as a break a number of elements indiscernible from one another, we say the continuum is of one dimension. If, however, the break consists of a system of elements forming among themselves a number of continua, we say the original continuum is of many dimensions. When the break forms more than one continuum, the original continuum is of two dimensions. If the break is a continuum of two or more dimensions, we say the original continuum is of three dimensions; and so on. In plain English, a linear continuum in space is one which may be broken by a point, a plane continuum one which may be broken by a line, a volumetric continuum one which may be broken by a plane; and so on. Concerning the psychology of space, this is explained in the usual manner, emphasis being placed on the motor adjustments.

The treatment of the physical sciences is rather cursory, as one might expect from the amount of space devoted to it. The empirical and tentative character of our various laws is shown, so M. Poincaré states, in the changing values and new interpretations necessary for the various principles, those, among others, of Carnot, Newton, Lavoisier, Mayer, etc.

The latter part of the book (Part III.) is taken up with a criticism of a certain M. le Roy, while at the end of the book, science is defined as 'a system of relations' (p. 266). Science does not lead us to know the true *nature* of things, but rather the true *relations* of things (p. 266).

I have attempted to touch upon only certain more or less interesting parts of the book in a suggestive manner. It requires close reading and rereading to get all there is out of it. And the large field which it tries to cover presupposes a varied and extensive knowledge at the start.

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Esquisse d'une théorie biologique du sommeil. E. CLAPARÈDE. *Archives de Psychologie.* Tome IV., 1905. Pp. 245–349.

The various physiological theories of sleep are first reviewed, some of which are classified by the author as 'hypotheses of mechanism,' others as 'autonomous theories.' Under the former head falls the widely accepted theory of *cerebral anæmia,* well known from the writings of Mosso, Howell and others; likewise the directly opposed theory of *cerebral hyperæmia.* In the same class, also, is placed Duval's ingenious hypothesis of the *retraction of the neurones,* according to which sleep and other dis-associative mental states have for their cause a severance of continuity in the neural paths, resulting from a retraction of the dendritic processes. Against these and some other views which he cites, the author offers the following general criticisms:

1. The supposed facts upon which the theories are based are not sufficiently well grounded, and the testimony of various writers is in some cases quite contradictory, as witness the uncertainty whether the brain is in a condition of anæmia or hyperæmia during sleep. Recent histological studies (*e. g.,* those of Apâthy, to which no allusion is made) have likewise discredited the retraction hypothesis.[1]

2. These phenomena, which are adduced as *causes,* might just as well be regarded as the *effects* of sleep.

3. Even supposing these phenomena to be in reality the immediate causes of sleep, we are not told *why,* for example, these changes in the circulation occur, or *why* the retraction of the neurones, etc.

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[1] Temporary *functional* interruptions in the continuity of nerve paths must, however, be assumed, almost of necessity; and such functional interruptions would, of course, furnish as good a physiological basis for sleep as the temporary *organic* discontinuity of Duval's hypothesis.